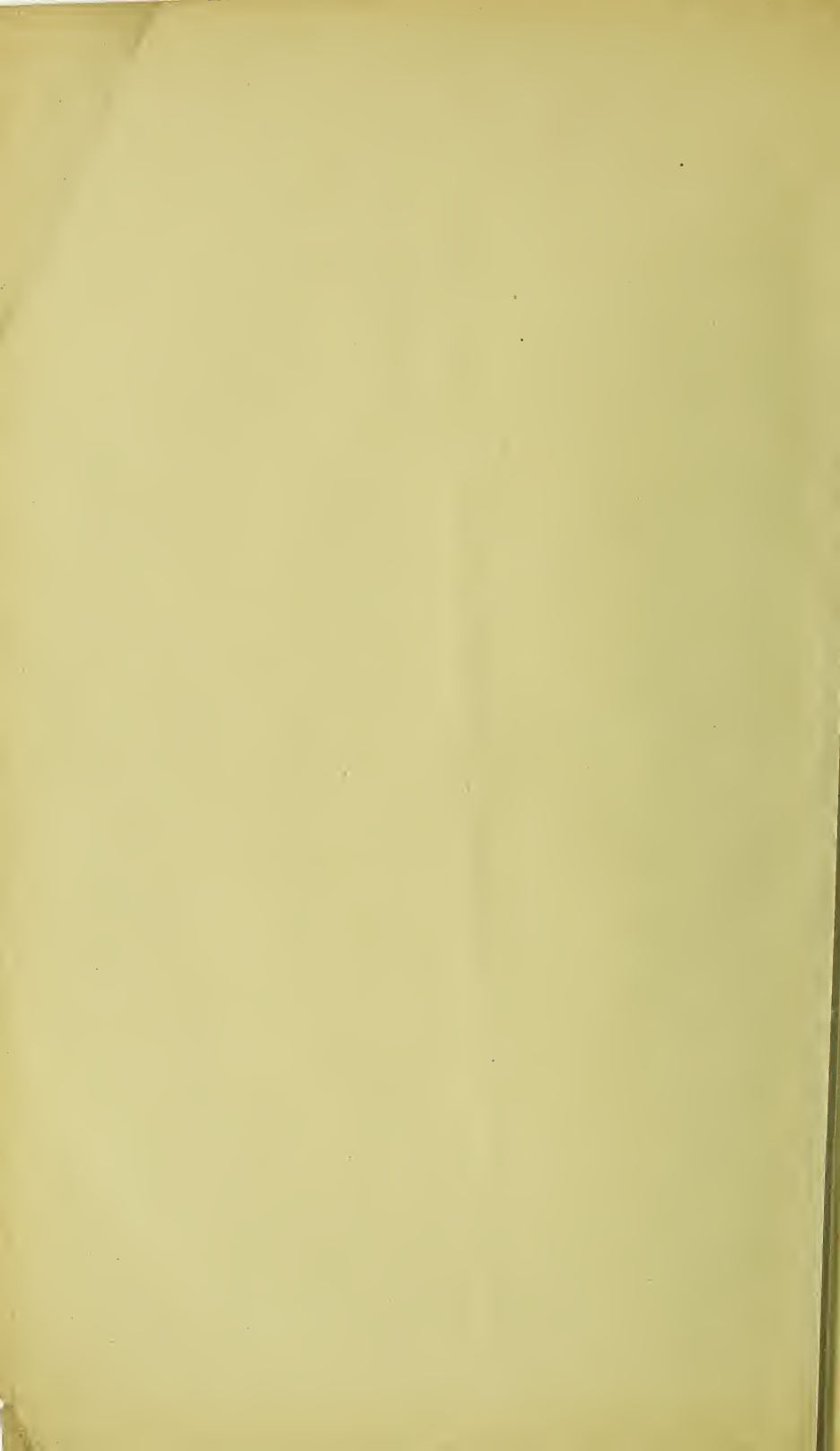


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With the Authors Compliments

WILLIAM PULTENEY ALISON:

HIS WORK IN RELATION TO HARVEY:

BEING

THE HARVEIAN DISCOURSE TO THE EDINBURGH SOCIETY,

APRIL 12, 1877.



BY

ALEXANDER HARVEY, M.D.,

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WILLIAM PULTENEY ALISON.

“The full interpretation of this great discovery is even now not given. For we are still learning what the blood does in that marvellous circuit which never ends, yet ever recommences.”—PARKES, *Harveian Oration*, London, 1876.

“The fluids of living bodies are liable to movements, and variations of movements, peculiar to their living state, but independent of any impulses which they receive from the solids.”—ALISON, *History of Medicine*, in *Cyclopædia of Practical Medicine*.

GENTLEMEN,—My first duty is to thank you for the high honour you have done me in electing me President of this Society for the year now current. And I thank you very heartily. I owe that honour, I know, to the name I bear—the name of the immortal Harvey—the discoverer of the circulation of the blood. Would that I could say that his blood, or the blood of his family, was the same as my own, ran in my own veins and in the veins of my children. Then, indeed, I should be proud: more proud I might well be than were I the descendant of a prince, and had blood-royal in my body. Peradventure, the Harveys of Kent are of the same stock—of the same kith and kin—with the Harveys of Aberdeen. I do not know. I wish they were. For then, apart from my own personal honour and glory, we might all of us here present claim him, in part at least, for Scotland. Be this as it may, let us not grudge England her claim to him as her son, and one of her sons whose name will endure for ever.

In casting about in my mind for a theme whereon to address you on this festal occasion, the 299th anniversary of Harvey's birth, I have resolved to make Dr Alison the subject of it. I am now aware, indeed, that, eleven years ago, Dr Halliday Douglas, then President of this Society, made him his. I was not aware of it, however, not only till after I had made election of Alison for my

hero, but had put together the greater part of what I am about to say to you regarding him. Yet it so happens that I shall not in the least trench on the ground Dr Halliday Douglas then occupied. Alison, too, will well bear a second hearing. He was a man so many-sided as to leave scope and verge enough for two of your orators.

The first heading of this address, as given in to one of your secretaries, was, "Jottings of Alison's Work in relation to Harvey, and to the Progress of Medicine in his own day." I called it "jottings," for of labours so manifold in his own profession as were Alison's, and of writings so copious as his, jottings was the fitting word to use for all that could be said of his work in a short address. But as I proceeded with my task, I found these jottings growing unduly upon me. On putting my sheets together, I found that they would occupy, not three-quarters of an hour only, but several hours, to the utter spoiling of this festival every way.

Your secretary—to my horror at the time—struck out the word *jottings*. I am now, however, well content that he did so, because, restricting myself, as I am obliged to do, to Alison's work in relation to Harvey, and to this alone, I purpose doing that great man all the justice I can. And a worshipper as I have been of him these five and forty years, it is a peculiar satisfaction to me to think that, without straining a single point,—that using only the plain words of truth and soberness,—I can, in the matter of the circulation of the blood, link Alison's name with Harvey's—ay, and in the sense indicated by Dr Halliday Douglas, as a *discoverer*, too. Suffer me this assumption, meanwhile—I hope in the sequel to make it good. I wish, indeed, that, before passing to my proper theme, time would allow me to say something of the man, and of his work generally; but it will not. I had put together some thoughts regarding him which I should like to give expression to. But I must forbear. Yet, I cannot altogether forbear. I must say a word or two of his work generally. The grand old man himself I shall remit to your own recollection of him as he went in and out among you, and as he taught you over the way.

In 1831 there appeared his "Outlines of Physiology;" and, in 1833, his "Outlines of Pathology." These two works may be said to have done for his time what Cullen's "First Lines" and John Hunter's "Treatise on the Blood," etc., did for theirs. Fifty years or thereby had meanwhile elapsed. Some systematic work was needed to gather up into one the fruits that had ripened in the interval. Those "Outlines" supplied the want admirably. Bichat in the early part of the century had laid the foundation of a new school in physiology, and in anatomy also. Cuvier followed; and he reared a grand superstructure in both, in the "comparative" direction; and Majendie and others in France and Germany made substantial contributions to the former. In Pathology, Laennec, Louis, Andral, Broussais, and others, also in France and Germany, had made large additions to our knowledge of diseased actions and

morbid structures. In this country, Sir Charles Bell, Sir Benjamin Brodie, Mr Shaw, Mr Mayo, and others, had been busy in the department of Physiology; while Dr Abercrombie, Dr Bright, Dr Blackhall, Dr Bateman, Dr Armstrong, Dr Hastings, Mr Travers, and others, had been equally so in that of Pathology. In Ireland, Dr Cheyne, Dr Percival, Dr Graves, Dr Stokes, and others, had been fruitful workers. The first quarter of the century had altogether added much to our stock of knowledge. But where was it all to be had in a concrete or systematic form, in our own tongue? Virtually, or to any good purpose, nowhere,—nowhere at least for students. In Physiology, indeed, we had Blumenbach's Institutions, and Majendie's work,—both of them translations from the original; and, after a time, Bostock's and Mayo's treatises—valuable in their way, yet deficient in many respects. In Pathology there was really nothing, save Cullen's "First Lines," and his "Nosology,"—then the text-books in our medical schools, and in use at the examinations of the Army Medical Board.

At the time indicated, Alison struck in with his two volumes, giving us in a compact and condensed form a complete and truly scientific summary of what had been done up to the time at which he wrote. Singularly rich they were, and exhaustive in the exhibition they presented of facts ascertained. Nor were they less conspicuous as a studied exposition of the principles underlying the facts—a feature pointedly referred to by him in his prefaces as all-important, yet too much neglected. I was myself a student at the time; and I well remember the intense satisfaction they gave. Students felt that, in view of the pass-examinations before them (less stringent then than now, and having more regard than in these days to the capacity of the human mind), they could breathe more freely; and that, in respect of the grand fundamentals of their profession,—to wit, "the functions of the living human body as performed during health, and as altered by disease," they had got something very reliable and very precious; while, with an imperfect system or science of therapeutics, imperfect still and uncertain, the *Vis Naturæ Medicatrix* was then—as now, in this country, as ever and to this day in China and the East, and in Africa and the islands of the sea—the great safeguard of the community. Would that Alison had given us, as he hoped to do, an exposition of the diseased functions of the living body, "as influenced by remedies;" nor this alone, but also of those diseased functions as righting themselves spontaneously or independently of remedies—that is, of the provisions of nature for the spontaneous cure of diseases, and the processes by which the morbid or abnormal changes, dynamic and organic, revert back of themselves to the normal and the healthy. The great *desideratum* in these days is a system of *Natural Therapeutics*. On this alone can we hope to build any satisfactory system of *applied* therapeutics. We must know what Nature, *unaided*, can do, and what she

cannot do, in the cure of disease, before we can rear a system of what I have called applied therapeutics, or draw sound conclusions as to the real action and the intrinsic value of our remedial agents. What we need above all things is, an exposition—a full and a detailed exposition—of the *Vires Naturæ Medicatrices*.

The time at which those two volumes appeared was a remarkable and a memorable one. It was the era of the Reform Bill, and of other great political changes. There was then an extraordinary activity in the world of mind in all departments of thought and action. And those two tiny octavos soon passed into oblivion. No second edition of the Pathology was ever asked for, albeit the Physiology happily reached a third—happily, I say, for it contained a singularly valuable additional chapter—the second, “On the most General Laws of Vital Action.” But those two volumes did their appointed work. They were themselves rather too strong meat for most students. They needed close application—some considerable effort of attention—like Butler’s Analogy and his “Fifteen Sermons.” Yet, as I have said, they did their appointed work. By means of them, and by his oral teaching, Alison gave a fresh impulse to inquiry—scientific and practical. He taught others; and he taught them so as to make them supersede him. Carpenter was Alison’s pupil, and (avowedly?) gained his inspiration as a physiologist from him. Sir Thomas Watson was also his pupil. And both these distinguished men, coming after him, yet working contemporaneously with him, supplanted him, or they more immediately do so. Dr Carpenter, acknowledging him as a prince among physiologists, apologizes, in the dedication to him of his great work, “Principles of Human Physiology,” if he has ventured to differ from him on some points. And are not the “Outlines of Pathology” reflected in almost every page of Sir Thomas Watson’s incomparable Lectures? Let me not be misunderstood here. Not that these Lectures could not have been written without the Pathology. For Sir Thomas, himself a master in Israel, had himself been working independently in the same rich and deep mine. It is simply this, that Watson was manifestly much beholden to Alison. And thus, having in those two works of his served well his day and generation, he gave place to those two eminent men, and to others. He stood aside; while yet he continued to enrich the science of Physiology and Pathology with not a few valuable contributions. So far forgotten he was, while still highly honoured and greatly esteemed. And, in my opinion, he was somewhat unduly forgotten. For both these works of his will well bear a reading, and a fruitful reading, in these our own days. Yet, in these days, students are so jaded with work that they have no time for the reading of other than their class text-books, and the medical journals; and, directly they “pass,” there comes to most a reaction from professional reading of the higher sort.

One word more as to Alison's style as an author. It has been spoken of as too condensed, as parenthetical, and even in some degree obscure. His writings require, indeed, as I have said, close attention. And parenthetical they sometimes are. Yet this has to be borne in mind, namely, that he had no time to revise what he wrote. For at least twenty-five years of his life he was always writing, while yet he was constantly occupied with professional and other work, and spending himself, too, in abounding labours of love and duty (self-imposed indeed) among the poor. Altogether, the amount of his writings was—simply immense. Then consider further, what must needs have come upon him daily as a professional man (as the care of all the churches came daily upon St Paul)—namely, a continual breaking in upon his time. All this allowed for, and the amount of reading and research, together with a corresponding amount of thought and reflection, also allowed for, I think it will be granted that his work as an author was admirable. There is a singular directness and precision in his style, a singular richness in his matter. To my own mind, there is even a charm in his style; and did time allow me, I could cite not one, but many expressions, sentences, and whole paragraphs of great beauty, quite as deserving of being taken into the category of household words, and into a collection of "elegant extracts," as those of almost any other of our distinguished writers. Those that are to any extent familiar with his multifarious writings must be struck, I think, with the wealth of his mind, with the great extent of his information, with the firm grasp he took of what he gave his mind to, and with his judicial exposition of it all. Imagination he had in a high degree, a far reaching insight into things; and (as Halliday Douglas remarks) he was at once fearless and logical in his judgments. Moreover, the habit of his mind was no less practical than scientific. There was nothing of the book-worm in him. He did not give us "great heaps of facts" of no use to anybody. Dr Andrew Duncan, jun., and Dr Craigie, his contemporaries, probably knew a great deal more than he did. But it may be doubted whether they had his practical insight, or his great grip of principles.

All this as to Alison I stand by. Of the men of his day between 1825 and 1845 (or 1820 and 1845), he was, I hesitate not to say, *facile princeps*,—the foremost man of his day in Edinburgh; and when I say Edinburgh, I mean in the United Kingdom. And I say this with a full admission of the fact (for such it seems to be) that since then, or before the expiry of that time, London men were coming to the front, and now stand in the front,¹ albeit some of them imported there from Edinburgh, or educated in Edinburgh.

¹ I have long and often thought that the *Sydenham Society* would do a real service to the profession, and especially to the younger members of it, were it to gather up into two or three volumes the more important of Dr Alison's miscellaneous medical writings, reviews, etc. Were it only to serve the purpose to the mind (to use a favourite quotation of his own from Bishop Berkeley) that a certain sort of crop does to the soil,—one raised "not for the sake of the

Pardon me, Gentlemen, these preliminary remarks as to Alison. I feel that I cannot help making them. The number of those that well knew him and his work, as I did, is rapidly lessening. And soon, of those that knew him personally, there will be none left to speak of him. One needed to know him living to appreciate him fully. I had the happiness to make his acquaintance in his best days, and to maintain this acquaintance by correspondence to the very end of his life. And it will ever be a pleasing thought to me that he regarded me as a personal friend. Let me add, in a single word, that I have been led to embrace this opportunity of giving expression to my estimate of him by the reflection that he has not yet received either from the profession or from the public the consideration that is his due.

And now, without further preface, I come to speak of Alison's work in relation to Harvey.

What was it in respect of the Circulation of the Blood that Harvey discovered? The entire circuit of the blood in the system, its course or movement as a whole. Before his time something had come to be known as to parts or portions of this movement. Servetus, Colombo, and Cesalpini, who lived in the sixteenth century, had, it would appear, each of them, tolerably accurate notions of the circuit of the blood through the lungs. They seem also to have known that in these organs the blood is somehow acted on by the air. But beyond this they not only knew nothing, but their ideas were positively erroneous. In the century following, some time about the year 1616, Harvey, by a course, a sustained and prolonged course of careful observation, experiment, and induction, made out the actual movement of the blood throughout the entire organism—through the system as a whole. What before was known of the circulation was at once partial as far as accurate, and erroneous as to the rest, and the main part of it. Harvey made our knowledge of its circuit complete and accurate, and he made it clear to demonstration. As Dr Parkes remarks, he “proved the truth of his doctrine so clearly, and by such able arguments, that even at the present day one wanting to prove the

fruits, but to be ploughed in as a dressing to the soil,” and for the enriching of it,—such a collection would be of great value. And here I would take occasion to remark, as I shall find no other place for it, that in his review (in the *Edinburgh Medical and Surgical Journal*, vol. xxvi.) of Travers on “Constitutional Irritation,” we have the substance of his chapter in the “*Outlines of Pathology*” reproduced in the “*Outlines of Pathology and Practice of Medicine*,” on “Cases of Sudden and Violent Death.” The review and chapter together are worth their weight in gold. Thoroughly original in its whole conception and treatment that chapter is, and the review no less. And the study of both, as dealing with the border land between Physiology and Pathology, as treating of what Alison rightly calls “the simplest cases in Pathology,” may be said to form the fitting introduction to the study of the more complex cases that constitute DISEASE. How well the late Dr P. M. Latham appreciated the value of that study appears from his inimitable “*Lectures on Subjects connected with Clinical Medicine*” (1836), pp. 125-7.

circulation of the blood must adopt Harvey's method." One thing only he failed to demonstrate otherwise than inferentially, to wit, the actual passage of the blood through or along the capillaries from the arteries into the veins. But the existence of such minute vessels was then unknown, and the optical instruments then in use were inadequate to exhibit to the eye the motion of the blood in these vessels. He "lived before the days of microscopic observation and fine injections." With what deliberation he carried on his researches appears from the fact, that, although he had made good his theory to his own satisfaction in 1616, or before this time, he continued to pursue his inquiries for twelve years longer before promulgating that theory to the world. A rare forbearance this, as Dr Bostock remarks!—Might he not have feared that some one would anticipate him in his great discovery. But in those days men thought twice before venturing to kick against the pricks. Had he lived in Italy or in Spain, the revelation of his discovery might have brought him to the stake.

Then, as to the power or powers concerned in effecting the movement of the blood, Harvey knew that the main power and the essential power lay in the heart. Probably he knew nothing beyond this,—probably he thought of nothing beyond this. But just as he knew nothing of a system of capillary vessels, so he knew nothing of what goes on in them between the blood and the tissues, and could conceive of no power operating there, either in the vessels themselves or in the changes of which they are the seat, as coming in aid of the heart's action in maintaining the general circulation.

The first additions made after Harvey's time to our knowledge of the circulation was the ocular observation by Malpighi of the passage of the blood from the arteries into the veins. This was about the year 1660 or 1661, or fifty years later than Harvey's discovery was completed. What Malpighi saw was in the web of the frog's foot, in the membranous part of the lungs, and in the mesentery. With his eye, he saw "the arteries rapidly projecting the blood in successive waves towards their extremities,—the veins receiving the blood, and returning it in a uniform stream to their trunks." It would appear, however, that he only saw the mere passage of the blood through the smallest arteries and veins; and this further, namely, that the pulsation is confined to the arteries, and does not extend to the veins. The rapidity of the blood's motion, and the interlacing of the vessels one with another, scarcely permitted him to detect with the eye the exact course it takes.¹

Nearly thirty years elapsed before any further advance was made in this direction. About the year 1698, Leuwenhock, with the aid of the microscope, saw the circulation in the capillary vessels as it goes on in the wing of the bat and in the tail of a fish. Since then this phenomenon has been familiar to physiologists—known to them in all its details.

¹ Bostock's Physiology, 2d ed., vol. i. p. 347.

The next step in advance was the discovery that the smaller arteries—on to the smallest that can be discriminated as such—are possessed of the vital property of *contractility*. The researches that led to this discovery seem to have begun by Haller. They were afterwards more fully prosecuted by John Hunter, by Verchuir, Dr Wilson Philip, Dr Parry, Dr Jones, Dr Thomson, Dr Hastings, and others. The kind of contractility possessed by these vessels was ascertained to be that of slowly responding to a stimulus or an irritant directly applied to them or to their coats—the contraction or narrowing induced being permanent for a time, and then slowly relaxing before the vessels resume their normal calibre. And to this form of contractility the term *tonicity* was applied. It differs widely, as you know, from the contractility exhibited by the heart, in which the process of contraction, rapidly induced, is as rapidly followed by relaxation. Still further: this attribute of tonicity came, subsequently or contemporaneously, to be associated with the possession by the smaller arteries of a coat essentially *muscular* in its structure.

This ascertained, physiologists were naturally led to inquire whether, in virtue of this property inherent in them, the arteries may not come in aid of the action of the heart in carrying on the general circulation. And Haller “adduced a variety of considerations to prove that the contraction of the arteries is an important agent in the circulation of the blood” (Bostock, *ut supra*, vol. i. p. 393). So also did Blumenbach. He could not see how otherwise the circulation could be carried on in the vena portæ or in the placenta.

This question we shall have to consider presently in connexion with Alison’s views. But just at present I would take occasion to observe, that in discussing this question, physiologists have not always been sufficiently careful to discriminate between two things,—to wit, on the one hand, the general circulation, and in connexion with this, the general action of the arteries; and, on the other hand, partial or local aberrations in the movement of the blood, and these in connexion with increased or decreased action of the arteries in the parts concerned. And they have not unfrequently imported discussions regarding the one into discussions regarding the other, and in such manner as to breed real confusion. Even Alison, following herein the example of his predecessors, in discussing the question whether any, and, if any, what additional power comes in aid of the heart’s action in carrying on the circulation, mixed up the one with the other. Had he avoided, as he easily might, mixing up these two things, he would, I think, have done himself greater justice in his exposition of this department of Physiology, and placed the whole subject on a better because a clearer footing.

To proceed: Dr Alison first shows, as is easily done, that the action of the heart is *essential* to the motion of the blood in all parts

of the body. As to this, let it suffice to say, that on the cessation of the heart's action the circulation of the blood comes instantly to a stand everywhere.

He next makes it his business to show that the action of the heart, although essential, is not the sole or the only agent concerned in that movement. He strives to show that, whatever be its nature, or however it may operate, some other power, cause, or condition is equally essential with that of the heart in the maintenance of the general circulation. And he finds the *crowning* proof of it in one single yet decisive fact,—“the striking fact,” as he calls it,—namely in this, that when the aeration of the blood in the lungs is suspended, completely and at once arrested, the whole circulation comes almost at once to a standstill,—first in the lungs, and next, and necessarily, in the whole system. He might well have put this fact and proof in the foreground of his argument, instead of leaving it, as he does, to the end of it.

Now, as far as I know or can discover, Dr Alison was the first to adduce this fact in proof of the assumption that there is a power, or that there are powers, equally essential—I do not say *alike* essential, nor does he—but equally essential with the heart in carrying on the circulation. I can see no reference to the fact in question, with that special purpose in view, in any physiological or other medical writer before him. And it is clear that to him the proof was decisive. Attempts have been made, I know, to show that the fact is not of the value he imagined. But, supported as it is by a large body of evidence then and subsequently adduced by him, and collateral evidence, towards the gathering of which he paved the way, it cannot, I am persuaded, be gainsaid. Nothing can more strikingly present the fact to the mind, or the import of it, than a well-known experiment of Bichat's.¹

This proof, adduced in support of the assumption that there is an agency concerned in maintaining the circulation of the blood distinct from that of the heart, and equally essential thereto as is the action of the heart, and that assumption allowed,—meanwhile, at least, the question arises, What is this agency, what this power, what its nature, what its mode of action?

Dr Alison takes up and pursues this question. The supplementary power concerned obviously resides at a distance from the heart. It clearly lies in, or in the immediate neighbourhood of, the capillary bloodvessels. Is that additional power, then, the vital contractility of the smaller arteries, in *act*? Does it reside in these minute vessels? Is it in any way connected with them

¹ “If a stop-cock be adapted to the trachea, exposed and divided, and afterwards it be shut, the blood becomes darker, and springs for a short time with its usual force” from a cut artery, “but at length the jet becomes gradually feebler. *Readmit* the air, the blood becomes almost immediately red; its saltus also rapidly increases.”—*Sur la Vie*, etc., p. 222; quoted from Dr Kay-Shuttleworth's paper in *Edin. Med. and Surg. Journal*, vol. xxix. pp. 41, 42.

and their known property of tonic contractility? Dr Alison maintains, and he demonstrates conclusively, that it has nothing to do with the exercise of that power inherent in these vessels. He shows that, while this power may, and does by its exercise *affect* the motion of the blood in *individual* parts, or locally, it is of such a nature that any *general* exercise of it, that is, that its equable exercise everywhere, or generally throughout the system, could have no other effect than that of retarding, impeding, opposing, instead of promoting, the general movement of the blood onwards. This inference, besides being conclusively established by sundry experiments and several considerations referred to by him, is abundantly clear on a moment's reflection.

In point of fact, it is in one instance only, it is on one occasion only, that this inherent vital contractility of the arteries comes in aid of the action of the heart; and that it does so *then* is a matter of certainty. It is the very last act of that organ, it is in the *articulo mortis*. The heart in this its last act sends out the blood that is in it. The aorta and the larger arteries help also by their elasticity to propel it on. And the smaller arteries everywhere, having now nothing behind them to keep them to their ordinary or wonted calibre, gradually contract upon themselves and squeeze out what of blood remains in them into the veins. This, I repeat, is the only occasion on which, in virtue of their contractility, the smaller arteries can or do come in aid of the action of the heart, in respect of the general circulation. And Dr Cullen, in as far as he understood the property of these minute vessels, made their general action throughout the system (in a morbid sense) the basis of his theory of fever. Calling it a *spasm* of the extreme vessels, this spasm leads to an increased action of the heart in order to the resolution of it. He seems sufficiently to have understood the *necessary* effect of any general exercise of the vital power of these vessels.

What purpose that property of tonic contractility possessed by the smaller arteries serves in the animal economy it does not lie to my hand to consider. Suffice it to say, in relation to Dr Alison's teaching, that it has neither part nor lot in the matter. It has nothing to do in carrying on or in maintaining or sustaining the general circulation. Its general exercise could have but one effect, that of opposing, impeding the action of the heart. Inject an irritant fluid into the aorta of a newly-killed animal, while that property of contractility is still extant in the vessels, the fluid will be arrested in its course.

Nay, more, Dr Alison ends this part of his inquiry by throwing the bloodvessels overboard. As mere canals, indeed, or carriers, they bring the blood into relation with certain other parts than themselves. As such they bring it under the dominion of certain other laws than that of contractility,—laws and parts which he has in his far-seeing eye. They are laws, he adds, by which the movements—observe what he says, the *movements*—of the nourishing fluid are

effected (not *affected*, observe, but *effected*) in those animals that have *no* heart,—nay, *no* circulating system,—and—and in *vegetables*. Here, as regards the agency or the power now in view, the blood-vessels are put out of court altogether.

Wherein, then, lies this power, or these powers, that are independent of the bloodvessels, but which come in aid of the action of the heart, and are essential to the maintenance of the circulation in all parts of the living body? What are those laws, under the dominion of which, to use Alison's own words, the blood is thus brought? It is, again to use his own words, as we have them in the first edition of his "Outlines of Physiology," it is "*the vital affinities*,"—affinities "which regulate the chemical changes that are wrought on the blood in the capillaries." Permit me here to remark that, along with the chemical changes there wrought, Dr Alison might and ought to have coupled the *plastic* changes, more important, agreeably to his own teaching, and of far greater magnitude than the chemical, in the animal body. Perhaps, however, he used the word chemical as embracing the plastic also. But this by the way. It is this power of vital affinity that is, or that furnishes, the additional or supplementary agent in maintaining the general circulation. This conclusion he reaches after sundry considerations which he adduces, and, very particularly, on reflecting on the fact that on "minute examination of the progressive development of parts in the foetal state of different animals, it appears that various parts are in progress of organization (implying regulated movements in the fluids) before the heart exists, or the arteries are distinctly formed." This argument he brings forward, after having shown the utter insufficiency of the vital contractility of the arteries in the maintenance of the general circulation. And then, having adduced this very powerful, and indeed in itself conclusive argument, he clenches it by a decisive reference to the fact (already adverted to) that "when the chemical changes at the lungs are arrested by obstruction to the access of air thither, the contractions of the right ventricle, although continuing vigorous for a time, fail of their wonted effect, and the blood stagnates in the capillaries of the lungs" (page 43). With nothing directly interfering with it, the right heart fails to drive the blood outwards. It fails because of some cause operating in the capillaries into which it has pumped the blood. This cause is simply the arrest of the chemical changes going on there between the air and the blood. Should this arrest be permanent, the arrest of the circulation there and throughout the system will be permanent, and death will ensue. Yet, readmit the air within a reasonable period, those chemical changes will again be effected, the blood will again move on, and the action of the heart will be resumed.

To that power of vital affinity to which he ascribes the aeration of the blood, he subsequently refers also (in another part of the Outlines) all the phenomena of secretion, excretion, and inter-

stitial or molecular nutrition. These phenomena, he says, and says truly, "do not take place fortuitously or at random, but according to fixed laws," while they "are inexplicable by and inconsistent with any principles that can be deduced from the observation of dead matter, or of other functions of the living body." "We refer them, therefore, to a vital property, our notion of which is, as yet, necessarily vague and imperfect, which modifies chemical affinities in the living body; varying in different parts of the body, and causing these to be differently affected by, and produce different effects on the blood that pervades them, influencing, likewise, no doubt, the chemical nature and relations of the blood itself. To this property the best name that has been given is VITAL AFFINITY. Its existence will, no doubt, always be an ultimate fact in physiology; but the limits of its agency, and the laws according to which it modifies the chemical relations of the substances subjected to it, may no doubt be ascertained, and their development will probably constitute the next great discovery in this science" (Outlines, 1st ed. p. 73).

And so in a manner it did in other hands, his own not wholly excepted. This generalization, however, was, as far as I can make out, Dr Alison's own. And a splendid one, I venture to say, it is. It seems, indeed, so obvious that one may say in this matter, Alison taught us nothing but what we knew before. Why then did not the physiologists that went before him at least refer to it, indicate it, name it? But it is the old story, so pithily put by Archbishop Secker: "Truth *proposed* is much more easily *perceived* than, *without* such proposal, it is *discovered*." Ay, there's the rub. It is easy to see a thing put plainly before us, and yet we might never have found it out for ourselves. Rather should the exclamation here be, "Dear me, how simple it is! why did I not think of it myself before!" I say that generalization was Alison's own. And Professor Gairdner, I see, in a very concise and beautiful statement of what he calls "Alison's physiological system," in his notice of that great man, credits him with it, sets it entirely to his account.¹ How the idea of that vital principle

¹ Obituary notice of "the late Dr Alison" in *Edinburgh Medical Journal* for November 1859, pages 475-76. But, indeed, even for that further view of this vital principle, which he speaks of under the head and by the name of Vital Attraction and Repulsion, Dr Alison claims for himself the credit of it. Remarking that "this principle of vital attraction and repulsion is more fully recognised by the physiologists of Germany than by those either of France or of this country," he goes on to say, "as I confidently expect that, notwithstanding the opposition it has lately met with, it will gradually make its way in physiology, I hope I may be excused for observing that almost all the facts stated in this volume in illustration of it were noticed, with the intention of establishing the existence and illustrating the importance of a principle affecting the motion and distribution of the fluids of living animals peculiar to their living state, and independent of the impulse or pressure of their living solids, in my lectures, and in a paper read to the Royal Society here, before the publication of the important works either of Müller or Burdach."—Preface to 3d edition of "Outlines of Physiology," p. v.

grew upon him, and with him, as years rolled on! what a flood of facts bearing on it he brought together in subsequent editions of his *Physiology*, and in his article or chapter on *Inflammation*, you all know. And you know also how it has received its development in the discovery of cells and cell-growths, and through the prosecution of histological research and of organic chemistry. That the principle in question is a real one, and that it is also a fundamental one in physiology, will not be disputed, except by those that do not go to the root of things in this science,—that occupy themselves rather with special details or departments of it, and have no fancy for what they may regard as abstract speculations,¹ or except by those that discard altogether the notion of vitality. With these last I have nothing to say, being myself a vitalist. But to those that admit vitality, I would humbly venture to submit, that they can no more do without this principle of Vital Affinity in explaining certain phenomena in the animal economy than birds can fly without wings.

This was the point reached, this the doctrine confidently promulgated by Dr Alison in 1831, in reference to the moving powers concerned in carrying on the general circulation. In my opinion he made good the point. He established his assumption that, while the action of the heart is essential to the maintenance of the circulation, there is yet another power in its own way not less essential thereto. He showed that this additional power does not reside in the arteries, and is not the property of contractility inherent in them; nay, that it is wholly independent of that property, and extrinsic of the bloodvessels; and, specifically, that it is the power of vital affinity by which the changes, chemical and plastic, that are constantly going on in the capillaries between the blood and the tissues are effected. Let me just remark here in passing, that this view had already culminated in his own mind in a new theory of *Inflammation*. This sufficiently appears from his *Heads of Lectures on Pathology*, appended to the first edition of his *Physiology*; and in a couple of years thereafter, that theory, the one now received among us, was fully developed in his "*Outlines of Pathology*." To this I will advert hereafter should time permit.

Meanwhile, suffer me to make a few further remarks on what I may call Alison's supplementary work in relation to Harvey.

Taking his stand on sundry collateral considerations, Dr Alison, as I have said, clenched the proof of his affirmation that the changes going on in the capillaries between the blood and the

¹ As to this, Dr Alison himself well remarks in one of his prefaces: "It seems to me that there is in many medical writings an indifference to the establishment of such general principles, both in physiology and pathology, as the information we already possess sufficiently warrants, which argues inattention to the value and importance of these principles."—Preface to "*Outlines of Physiology and Pathology*," 1833, pp. x. xi. And, no doubt, in making this remark, he had this principle of vital affinity specially in his eye.

tissues, and effected there by vital affinity, are equally essential with the heart in carrying on the circulation, by the fact of what obtains in asphyxia. A more decisive actual proof of it he could not, I think, have furnished. His theory of asphyxia, indeed, has, I know, been called in question; but to little purpose, I apprehend. It stands good to this day, and it can be substantiated beyond all cavil or dispute, by additional considerations which Dr Alison might have adduced, but did not otherwise than inferentially.¹

As to this, let me ask you to glance for a moment at what obtains in the capillaries of the general circulating system. Founding on what holds in respect of the pulmonic capillaries in asphyxia, Dr Alison might well have asked, and bade those that took exception to his inference from it make answer,—Supposing it were possible to arrest the processes of molecular nutrition, and of secretion, excretion, exhalation, and absorption in the general system everywhere and simultaneously, as it chanced to be possible to arrest the aeration of the blood in the lungs, do you imagine that the general circulation would or could then go on? For this would be the *parallel* case. Circumstances prevent such an observation being made, or an experiment of that kind being tried. But can any one suppose that the result would be different,—that such an arrest would not of itself, and independently of any agency acting on the heart, be followed by an immediate arrest of the circulation throughout the whole organism?

And the inference drawn by Alison from what holds in asphyxia comes to be all the stronger when we think of the simplicity, the absolute simplicity, of the changes effected in the lungs in the process of aeration, as compared with the complexity of the processes of interstitial nutrition and glandular secretion. In the case of the lungs, it is a simple affair of transudation and imbibition. The blood simply gives off carbonic acid; it simply takes in oxygen. There is no complex process of cell-growth as in secretion. There is no such process of disintegration of dead tissue and the reduction of this dead tissue into carbonic acid, urea, etc., as takes place in molecular nutrition. There is no building up anew of new living tissue by existing living tissue, to replace the old and the dead and the cast-off tissue. In the process of aeration there is a simple transudation and exit of carbonic acid, a simple imbibition or sucking-in of oxygen—nothing more.

¹ Assuming the general principle contended for by Alison to be sound, namely, that the arrest of the circulation in the pulmonic capillaries is primarily and essentially due to the arrest of the changes which naturally go on there between the blood and the air, a further question may be raised and may admit of solution, to wit, How is it that the arrest of the aeration of the blood leads to arrest of the circulation through the capillary vessels of the lungs? The solution of this question, however, would not invalidate Alison's theory. It would only explain or elucidate it. See, as to this, Dr George Johnson's Lumleian Lectures (Lecture I.) in *British Medical Journal* for 14th April 1877.

And yet the arrest of this simple process in the lungs brings the circulation to a standstill there. And it does so in a trice. Contrast this with the complexity of the processes that go on in the capillaries of the general system, go on there everywhere and incessantly, the processes of interstitial nutrition, secretion, and, in the case of the lacteals, of absorption. Can any one challenge the force of this larger yet parallel inference?

As to one of these processes, the most fundamental of all, and that to which, as far as the organic life is concerned, all the other organic processes are subservient, is that of *molecular or interstitial nutrition*. And as to this process I would submit, that neither in relation to the matter now in hand, to wit, the general circulation of the blood and the moving powers concerned in it, nor yet in relation to itself, do our physiologists (the late Professor Hughes Bennett excepted) treat of it in their writings as it deserves to be treated. They refer to it, but they do not analyze it, or put it in detail before their readers, or bring it prominently into view as that which furnishes the *raison d'être* of every other. Still less do they seek to refer it to its fundamental vital principle, that of vital affinity.

As it has a positive, and, indeed, a cardinal bearing on Alison's theory, and is but the analogue of that on which he mainly rested that theory, suffer me in as few words as possible to put the *essentials* of that great process clearly before you.

Long ago, yet not long after Harvey's time, a very distinguished prelate—Bishop Jeremy Taylor—gave expression to a fundamental fact in physiology, anticipating herein both Alison and Carpenter.

"Our very life," Jeremy Taylor says, "is nothing else than a succession of dying; every day and hour wears away part of it; and so far as it is already spent, so far we are already dead and buried."

A great truth this in animal physiology!—the foundation-truth, in fact! And beautifully put it is; expressed it is with a scientific precision wonderful for Jeremy Taylor's time. Yet, what a rare imagination Jeremy had! And our friend Alison herein following up Taylor, and anticipating Carpenter, has said, in regard to it, once and again, and in varying forms of expression: "All vital action is of exceedingly short duration in any living part." This, I say, is the foundation truth in animal physiology—in as far as relates to the "organic" life. No part of the animal body retains its vitality long. The *chemical* affinities by which the various organic compounds are formed, and the *plastic* affinities by which these compounds are transformed into the several organized tissues, and these again knit together and built up into the one whole body or organism, are exceedingly transient in their agency. Every particle, every molecule, every little bit of living matter thus formed within us, retains its vitality but for

a brief space only—a space, however, longer with some, shorter with other molecules. Each of these molecules may be said to have its own specific purpose to serve in the economy; and this purpose served, it straightway dies—dying, it may be said, in and by the act. Every muscular effort we make, every exercise of thought and will, exhausts the vitality of so much brain and nerve and muscle; and it does so, it would appear, in the exact measure and proportion of the acts of thought and volition and muscular exertion. Every part has some purpose to serve, some office to fulfil; and the great law is, that every effort and exercise of its function entails the extinction of its vitality, entails its death. We die daily, bit by bit, and thus dying, we are reduced to the condition of dead matter. This dead matter of our bodies is then disintegrated. It is acted on by the oxygen of the air we breathe; and, in the shape of carbonic acid, uric acid, urea, cholesterine, and sundry salts, it is cast out into the draught. There is no play or exercise, indeed, of vital affinity here. It is all an affair of ordinary (inorganic) affinity, albeit going on within the living organism. Nor can we doubt that in its own way and degree it is the source and the occasion of *a part* of the movement of the blood in the capillaries; and if so (curiously enough), it is an exception so far to the general principle so strongly insisted on by Alison, that none of the phenomena peculiar to living beings are explicable by a reference of them to the principles that regulate the changes going on in the inorganic world.¹

We die daily then; and the extent to which we do so may in a rude way be measured by the amount of the daily excreta, still better by the quantity of food and other aliment we take in daily. Thus thought of, the activity and the magnitude of this fundamental vital process will clearly appear.

We die daily then. Yet, as we continue to live, and to live on, a counterpart process to that indicated by Jeremy Taylor must also day by day go on within us—nay, hour by hour—incessantly. As we die daily, we must needs daily rise to life again. And so we do. The molecules that die inside us and are cast out of us must be replaced by new ones. If every day and hour wear away part of us, every day and hour must renew part of us. And this process of *renaissance* is in fact continually going on within us. And a wonderful process it is, when we come to think of it in detail—living muscle, living nerve, living bone, ere they themselves depart this life, severally building up anew, as by a generative act, from one common pabulum—the blood, new bone, new nerve, new muscle, setting them respectively in their right places, and endowing them with their own proper attributes.

¹ Those physiologists, however, greatly err that think that because they can form urea in their laboratory, a time may come when they shall be able to form albumen there also; for urea as formed within the living body is a product of organic matter already dead.

A great process this really is, the process continually going on within us, of dying and rising to life again! Little thought of it seems to be by some physiologists, perhaps because it goes on within us silently, insensibly, unconsciously to us, perhaps because it involves merely abstract details.¹

And a fact it is in relation to the question of the powers concerned in effecting the general circulation, or the movement of the blood in all parts of the body.

The fact, the experiment we may call it, founded on by Alison in his theory as to these moving powers, may be said to be a beautiful as it is a capital one, to wit, that as to the effect on the general circulation of the non-aeration of the blood. It is a beautiful one through special circumstances. The whole blood of the body has of necessity to pass through one single organ in every general circuit it makes; and the function of this one organ is absolutely under our control. We can at once put a stop to it, and then watch the effect of this; and we can set it agoing again, and take note also of the effect of this restoration.

A like experiment we cannot try with the general circulation. Obviously we cannot. Yet, judging from that on the lesser circulation, and keeping in view what has now been said as to the process of molecular nutrition, to say nothing of the complex processes of glandular secretion and lacteal absorption—a continual affair of cell-growth and of much beside—can any one doubt that, were it in our power simultaneously to arrest these processes, the entire circulation in the capillary bloodvessels everywhere would not be brought to a standstill, even *superstite actione cordis et arteriarum* for a minute or two?

This wonderful process has well been designated by a French physiologist—the “*turbillon vital*” of the living animal body. Very expressive that word *turbillon* is. What goes on in the ciliary frill of the muscle or oyster as seen under the microscope, the incessant rapid waving of the ciliæ, may give some idea of it. Put a sudden stop to that *turbillon*—can any one suppose that the general circulation would not be at once arrested? If so, should we not have in that arrest the exhibition of a power, not less essential than that of the heart itself, engaged in carrying on, and in maintaining and sustaining the general circulation?

The idea of this, and the demonstration of it, we owe to the genius of William Pulteney Alison. Neither in Blumenbach, nor Majendie, nor Bostock, nor Mayo, is the smallest reference made to this grand motive power.

The power in question, that of vital affinity, was regarded by

¹ The author hopes he will be excused observing here that the views set forth in the text in regard to this great process, and substantially as there advanced, formed the subject of his inaugural dissertation on Interstitial Nutrition, on occasion of his graduating M.D. at the University of Edinburgh in 1835.

Alison as a moving power. Clearly it was ; and as such we have, of course, been considering it here in relation to Harvey's work. In what sense, however, Alison regarded it as such, and how far he afterwards came to associate it with another power allied to vital affinity, yet seemingly in his mind so far at least distinct from it, because named by him separately from it,—namely, that of *vital attractions and repulsions*,—time will not allow me to consider. I content myself with this passing allusion to it.

But to complete Alison's views as to the agency of vital affinity, I must needs refer, however briefly, to another point. To this power of vital affinity he ascribed also all *local* aberrations or deflections of movement in the circulation of the blood, local congestions (normal and abnormal); and the reverse of this, the lessened flow of blood to parts. To him, the theories of increased or decreased action of arteries were of small value, indeed of none. *Ubi stimulus, ibi fluxus*, and the reference of this to vital affinity, its agency entailing movements of the blood this way and that, was simply another expression of his theory.

And his views here culminated, as I have said, in a new theory of *inflammation*. You are yourselves familiar with the old theories of that morbid process—how the main theory turned on the question of increased or decreased action of the bloodvessels. And you will remember, many of you, what debates we *did* have about this in our medical societies—about the question, “What is the proximate cause of inflammation?”

Now, as early as 1831, in his writings, and no doubt earlier still in his teaching over the way, Dr Alison clearly indicated that he saw the insufficiency of either or both of these states as furnishing an explanation of the *essential* nature of inflammation. In his *Physiology*, as we have just seen, he sets both of them aside as affording any explanation of even normal determinations of blood ; and he points to changes going on in the capillaries, but referable to the power of vital affinity, as the main cause or occasion of them. And in the *Heads of Lectures on Pathology* appended to that work, he says—“As it is very doubtful whether the action of vessels is the sole cause of the motion of blood in the capillaries (independently of the heart's action), so it is equally doubtful whether any alteration in their action is the sole cause of the different phenomena that occur in inflamed parts ; and it is certain that the changes wrought on the blood in inflammation cannot be explained merely by alteration of the force compressing and propelling it.” The one “very doubtful ;” the other “certain !”

This he said in 1831, and in the mere jotting of a lecture. Allow that then, like the man in the Gospel cured of his blindness, who saw men as trees walking, Alison had only caught a glimpse of the truth, one may say of that glimpse, what Cullen says of a single word introduced by Boerhaave (and after the lapse of forty years) into one of his aphorisms—“What a change of doctrine that points

at"—*points at*. What I have quoted from Alison points at a real revolution of doctrine: it was a mine sprung on the old theory. Carefully considered, one may see in those few words the actual germ of his theory as afterwards more fully evolved. In 1833, when his "Outlines of Pathology" appeared, he gave that theory to the world in all its fulness, and substantially as he gave it in his latest exposition of inflammation in his "Outlines of Pathology and Practice of Medicine," published in 1844. I have both his versions of it, his earliest in 1833 and his latest in 1844, now lying open before me, and I can see no essential difference between them. With Alison inflammation was, at both times, *a perversion of the process of nutrition*, referable to a perversion of the fundamental vital power of vital affinity.

The late Dr Hughes Bennett, if I mistake not, has put in a claim to something in the discoveries made as to the nature of that morbid process. For my own part I cannot discover wherein it lies. I have carefully compared Dr Bennett's exposition of it with Dr Alison's, and I cannot see wherein they differ. And Dr Alison had given his exposition of it to the profession before Dr Bennett (I presume) began his medical studies. Dr Bennett went perhaps more minutely into certain of the details regarding that process and its products than Alison did, and he used perhaps somewhat different words in his exposition of it. This, however, is a very small affair; and I do not see that Bennett added anything to what Alison had already told us as to its essential nature. In his primary reference of the whole process to an abnormal state of the fundamental vital process of *nutrition*, and this to an aberration of the power of *vital affinity* operating in this process, Dr Alison's theory lay. And Dr Bennett's theory is essentially the same—words perhaps apart. Some have claimed for the Prussians the credit of winning the battle of Waterloo. The Prussians came up indeed in force in time to slay the slain. But the battle was won—the victory achieved—when Wellington's memorable order was given and executed—"Up Guards and at them." How apt we are to be cheated by words! "*Credunt homines rationem suam verbis imperare; sit fit etiam ut verba vim suam super intellectum retorqueant.*" (This admirably expressed truth you will find, as a quotation, in Alison's own Inaugural Dissertation—*De Viribus Naturæ Medicatricibus*.) And I incline to think that it holds of any claim that may have been advanced by Dr Bennett in reference to this matter, or that may be advanced in his behalf, now that he is gone, by any of his friends.

And this brings me to the end of my task—Alison's work in relation to Harvey. Alison completed Harvey's work. He put the keystone to the arch of Harvey's work. Harvey, as I have said, had never seen the circulation in the capillaries, nor probably did he know anything of such vessels otherwise than inferentially. He knew nothing of the vital property of contractility inherent in

the smaller arteries, and could not have thought of them as having anything to do in carrying on and maintaining the general circulation. Still less can we suppose that he ever thought of, or thought of to any purpose, what goes on so incessantly in the capillary vessels of the general system, especially in respect of the fundamental vital process of nutrition. He knew that the heart is the great moving power concerned in carrying on the circulation; and he probably thought of no other. And, albeit, after him, physiologists came to be familiar with the capillaries and the circulation through them, and with the small arteries as being possessed of a power of vital contractility, no physiologist, as far as I know, till Alison arose, demonstrated, or clearly demonstrated, these two things,—*first*, that there is a power or a force at a distance from the heart that co-operates with the heart in carrying on the general circulation, and is equally essential with the heart in the maintenance of it; and, *secondly*, that this power has its seat in the capillaries, but is independent of them; that it arises out of the changes, chemical and plastic, that go on there between the blood and the tissues, and is referable to the power—a power first clearly indicated by him—of vital affinity. This was Alison's work in relation to that of Harvey; and a great work I venture to say it was. And the principle he reached here, Dr Alison applied to the explanation of sundry phenomena connected with the circulation, but embracing also those connected with the functions of nutrition, secretion, and absorption, phenomena as well normal as abnormal, and including a great pathological phenomenon, namely, inflammation.

And I conclude by saying, that to him, in this country at least, is unquestionably due the merit of being the first to establish both the reality and the importance of a *moving* power in living organisms, not dreamt of by Harvey, or by any of the physiologists that went before him; a power by which the movements of the blood and other fluids in the minute vessels, and in the cellular tissues of plants and animals, are determined, independently of the heart, or of a heart or other moving solids; a power dependent on the vital processes of nutrition, secretion, excretion, aeration, and absorption going on in them, and arising out of these processes. He was the first, too, to show how large a part that principle of movement plays in the morbid as well as in the healthy processes of nutrition and secretion. He it was that, on this ground, overthrew the old theory or theories of inflammation, and elaborated that now received. The generalization now indicated was a great and an important one. It was no less than the addition to physiology of a new FOUNDATION PRINCIPLE. In his beautiful discourse on Alison delivered from this chair eleven years ago, Dr Halliday Douglas remarks that Alison was not a discoverer in the ordinary sense, but that he was nevertheless a discoverer in another sense, namely, by *induction*,—seeing principles, inferences, conclusions, in fact, which their discoverers did not themselves see. How many of the highest

results in arts and arms have not had their root in discoveries of this kind? In respect of the circulation of the blood, I claim for Alison, on the footing stated by Dr Halliday Douglas, the credit of being a discoverer, and a great discoverer; and sure I am that had Alison been of the same stamp with some other physiologists, he would here have asserted his claims to be regarded as such. And as to his being on the eve of a discovery in another direction, I cannot forbear referring to a fact mentioned by Sir Robert Christison in his work on "Granular Degeneration of the Kidneys," namely this, that Alison was on the eve of anticipating Dr Bright. And so, had this happened, that disease of the kidney which bears Bright's honoured name, might now be known all the world over as *Alison's Disease*. Yet, ten chances to one if the dear Pulteney would have fared as well as Bright has done. And quite clear it is that his discovery of the power that supplements the action of the heart, in carrying on and in maintaining and sustaining the movement of the blood in all parts of the body, great although it be, and worthy of all honour, has not had the value it merits assigned to it. But it is of a piece with the little honour that has been assigned to him as a political economist. He deserved a statue here in Edinburgh—a city abounding in statues—for what he did for the poor, whom, as he said, quoting the words of the Master whom he followed, "We have always with us." Yet for this, as for other acts and deeds of his noble and disinterested life, we cannot doubt that great will be his reward in Heaven. "A sair saint" perhaps he was for Scotland, entailing on it a costly poor law. But a saint he was and a saintly man, and a great as well as a good man.

Would that Dr ACLAND, his literary executor and his intimate personal friend, who lived some time under his roof, and sat at his table, and knew him well, would yet give us an abiding record of the life and conversation of WILLIAM PULTENEY ALISON.

"His heart a mirror was, of purest kind,
Where the bright image of his Maker shined."
